LETTERS TO THE EDITOR.

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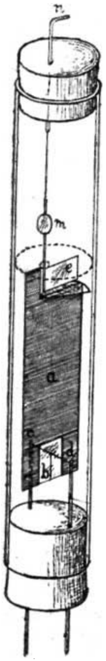
The Thomson and Peltier Effects.

A MODIFIED Joule radiometer has been applied successfully to show the existence of the Thomson effect in copper and the Peltier effect at the junction of two dissimilar metals.

The lower part of a vertical glass tube 3 cm. in diameter was divided into two compartments by a cardboard diaphragm *a*, 12 cm. long. A rectangular strip was cut away from the upper part of the partition, and a mica plate, *b*, was substituted for a similar strip from the lower part. A thin copper wire *cd*, No. 36, was soldered to two thick copper leads, No. 16, fixed into a cork. The wire *cd* was passed through a slit in the mica plate *b*, and the cork was pushed to within 1 cm. of the lower edge of the partition. The wire *cd* was arranged so that it was nearly bisected by the mica plate. A mica vane *e* attached to an aluminium wire was suspended by a quartz fibre within the aperture in the upper part of the partition. A horizontal disc of cardboard, shown dotted in the figure, was fixed to the upper cork by a pair of rods, and fitted inside the tube just above the vane. A horizontal sector-shaped plate of mica was fastened to the partition just below the vane to complete a channel for the current of air from the warm to the cool side of the partition. The clearance between the edges of the vane and the opening was about 1·5 mm., and the angular movement of vane was restricted by stops to about 30°. The motion of the vane was observed by reflecting a beam of light from the mirror *m* on to a scale at a distance of 80 cm.



The partitioned tube, together with the suspended vane, constitutes a Joule radiometer in which unequal heating of the columns of air in the two compartments will cause a deflection of the vane. The radiant heat from a candle flame at a distance of 2·5 metres falling on one side of the partition produced a deflection of 5 cm. of the spot of light on the scale. Owing to its high sensibility, it was found necessary to shield the columns of air from draughts and radiant heat from external sources.

A current of 6 amperes was passed through the thin copper wire *cd*, and a temperature gradient was thus established from the centre towards each end of the wire. The vane was deflected owing to the Thomson effects and the inequality of the Joule effects in the two halves of the wire. The position of the vane was fairly steady after an interval of five minutes, and the spot of light was brought to the centre of the scale by turning the bent wire *n* attached to the fibre. The current was rapidly reversed, and the spot of light moved quickly through 8 cm. on the scale. The wire *cd* was adjusted to slightly different positions in order to vary the inequality of the Joule effects, and practically the same result was found in every case. It was concluded from the direction of motion of the vane that in copper heat was absorbed when the current flowed up the temperature gradient.

To show the Peltier effect, an iron wire 4 cm. long and 1 mm. diameter was soldered to another pair of copper leads and bent just under the partition. The passage of a current of 1 ampere through the junctions was sufficient to move the spot of light off the scale. There is no need to reverse the current in this case, and it can be shown quite easily that heat is absorbed when the current flows from copper to iron.

F. W. JORDAN.

South-Western Polytechnic, Chelsea, S.W., May 8.

Compulsory Latin Diagnoses for Fossil Plants.

THE question whether descriptions of new species, genera, &c., of *fossil* plants should be accompanied in future by a diagnosis or diagnoses *in Latin* has been recently discussed among those who are working on fossil botany. It is not proposed here to enter into the arguments which have been advanced either for or against this proposition. It appears, however, that, so far as palæobotany is concerned, the arguments against the use of diagnoses in Latin are held far to outweigh those in favour of such diagnoses. In order to test current opinion on this point, a memorandum has been recently circulated by the writer among those who are engaged in the study of fossil plants in this country and in the United States, and by Prof. Nathorst in Sweden and Denmark. The object of the memorandum was to ascertain the present intentions of those working at fossil plants as regards this much disputed question. The result of this exchange of opinion has been very remarkable. Every palæobotanist in this country, in the United States, and in Scandinavia to whom a copy of the memorandum has been sent has expressed his intention of avoiding the general use of diagnoses in Latin, and, further, of recognising as valid diagnoses instituted in the future which are not published in Latin.

The memorandum in question contained two statements of intention, which were as follows:—

(1) "I do *not* propose to include a diagnosis in Latin in the description of any new species, genus or family that I may institute in the future, unless there appear to me, in particular cases, to be special reasons for so doing."

(2) "I will *not* refuse to accept new species, genera or families of fossil plants instituted by other workers in the future, solely on the ground that their description is not accompanied by a diagnosis in Latin."

The following is a list of those who have subscribed to both these statements. In the case of those names indicated by a *, some slight modification of the wording of one or other statement was made. These reservations follow the list of signatures:—

Mr. C. T. Bartholin, Copenhagen, Denmark.
Dr. M. J. Benson, Royal Holloway College, Englefield Green, Surrey.
Prof. E. W. Berry, Johns Hopkins University, Baltimore, U.S.A.
Prof. T. D. A. Cockerell, University of Colorado, Boulder, U.S.A.
Mr. W. T. Gordon, The University, Edinburgh.
Dr. Th. G. Halle, Stockholm, Sweden.
Dr. N. Hartz, Copenhagen, Denmark.
Dr. G. Hickling, The University, Manchester.
Mr. H. S. Holden, University College, Nottingham.
Mr. A. Hollick, New York Botanic Garden, New York, U.S.A.
Prof. E. C. Jeffrey, Harvard University, Cambridge, U.S.A.
Dr. R. Kidston, F.R.S., Stirling.
Dr. F. H. Knowlton, U.S. Geological Survey, Washington, U.S.A.
Mr. F. J. Lewis, The University, Liverpool.
Mr. A. J. Maslen, London.
Dr. H. Möller, Falun, Sweden.
Prof. A. G. Nathorst, Stockholm, Sweden.
Prof. F. W. Oliver, F.R.S., University College, London.
* Mr. Clement Reid, F.R.S., The Geological Survey, London.
* Mrs. Clement Reid, Milford-on-Sea, Hants.
Dr. D. H. Scott, F.R.S., Oakley, Hants.
Mrs. D. H. Scott, Oakley, Hants.
Dr. M. C. Stopes, The University, Manchester.
Mr. H. H. Thomas, The Botany School, Cambridge.
Mr. D. M. S. Watson, The University, Manchester.
Prof. F. E. Weiss, The University, Manchester.
Dr. D. White, U.S. Geological Survey, Washington, U.S.A.
Dr. G. R. Wieland, Yale Museum, New Haven, U.S.A.

Mr. and Mrs. Clement Reid have signed both statements with the addition of the words "if it is accompanied by a recognisable figure" at the close of the second statement after the words "diagnosis in Latin." They add,

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"we consider that the international character of the diagnosis will better be maintained if the Vienna rule be altered so as to read 'either a diagnosis in Latin *or a recognisable figure.*' This alternative would do away with the difficulty as to language, and would allow botanists to use their own language, provided they give a characteristic figure."

Prof. Seward has signed the second statement, and has added the following remarks:—"In view of the nature of much of the material available for investigation, I consider that it is undesirable to insist on a Latin diagnosis for all species described by palæobotanists. In cases where a formal diagnosis is possible, such diagnosis and a figure of the specimen ought to be given, but for the present at least I am not disposed to bind myself to the publication of a diagnosis in Latin. It is, I believe, in the interests of the subject to avoid pledging oneself to any fixed rule as regards either a diagnosis or the language in which the diagnosis is to be written.—A. C. Seward."

Prof. Zeiller, of Paris, to whom a copy of the memorandum was forwarded, has signed the second statement mentioned above, and has kindly expressed his reasons for being unable to subscribe to the first. These are as follows:—

"M'étant, au Congrès de Bruxelles, rallié, dans un esprit de conciliation, à la disposition générale qui fait de la diagnose latine une obligation, je n'en persiste pas moins à penser que cette obligation ne devrait pas être étendue à la paléobotanique, l'application s'en heurtant souvent, avec l'état fragmentaire et incomplet des fossiles végétaux, à des difficultés presque insurmontables, notamment lorsqu'il s'agit d'échantillons à structure conservée ne montrant que des caractères anatomiques internes.

"J'émets le vœu que tous les paléobotanistes s'unissent pour demander au prochain Congrès de leur laisser à cet égard la liberté dont ils avaient joui jusqu'ici, et à l'encontre de laquelle on n'a relevé aucun inconvénient.—R. Zeiller."

In conclusion, while, as we have seen, the British, American, and Scandinavian palæobotanists have agreed to avoid the *general* use of Latin diagnoses for the present, it should be pointed out that the object of the memorandum mentioned above has been solely to ascertain the opinions and *present* intentions of workers on fossil plants in this respect. It is, of course, understood that those who have subscribed to the two statements quoted above are not in any way bound as to the future, and they are at perfect liberty if, in altered circumstances in the future, they should wish to depart from their present opinions and intentions to do so. The view is widely held that perfect liberty in regard to matters of nomenclature, as in other directions, is essential to the progress of our knowledge of fossil plants.

E. A. Newell Arber.

The Sedgwick Museum, Cambridge.

Spitting Cobras.

The following note may be of interest, the more so as the existence of cobras in Borneo is denied in a recent work on Borneo ("Seventeen Years among the Sea Dyaks"). In your "Notes" in Nature of May 4 (p. 320) you refer to the "spitting cobras" of East Africa. The cobra of East Borneo also has the power of projecting its poison to a distance of at least 1 metre. In January of last year I was walking along a narrow jungle track about twenty miles from the coast, in lat. 1° 10′ S., when I saw a cobra erect preparing to strike at me. I struck it about 30 cm. from the head and broke its back (as I thought). It then projected two streams of liquid at me as I stood over it. A Bugis close beside me exclaimed, "dia menumpit!" (menumpit = to shoot with the blow-pipe). One stream struck the lapel of my coat, but I did not notice where the other struck. I placed the dead (?) cobra on a tree, intending on my return to carry it to my camp and bottle it, but when I returned it was gone.

About two hours after the incident I felt an irritation on my skin, which lasted for some hours, and then gradually disappeared. There was a newly healed leech wound at the place, but I could hardly believe that the cobra venom could have penetrated thick puttees, trousers, and socks all wet through. But I sent for the Bugis who had accompanied me, and without telling him anything, asked him to point out where the cobra poison had struck me. He immediately placed one finger on the lapel of my coat and another on my shin exactly where the irritation was.

He added that about ten miles away his brother had lost the use of his arm for three months because of a cobra which had projected its poison at him.

The cobra was black with a bluish sheen, but the throat was yellow. Estimated length, 1600 mm.

Boscombe, May 8. T. R. H. Garrett.

The Reform of the Calendar.

The article in this week's Nature on "Daylight and Darkness" leaves little to be said with regard to the so-called Daylight Saving Bill. Its adoption would indeed render us "the laughing-stock of the enlightened people of the world." No object, however good in itself, can be attained by a deceitful and underhand process, which must lead to many inconsistencies and misunderstandings. There is no reason why 12 o'clock should be the exact time of noon; in point of fact, it seldom is, as clocks must be regulated to keep *mean* time. But there is a very good reason, and it is of great importance, that the interval between two consecutive hours by the clock should be always exactly one hour. To regulate it otherwise would be deceitful and confusing.

My purpose, however, in this letter is rather with reference to Mr. Philip's letter on the "Reform of the Calendar." He denies that the week has had an unbroken continuance, because the paschal full moon on A.D. 31 (which he thinks was the year of the Crucifixion) fell on March 27, which was a Tuesday. Now the Jewish Passover had nothing to do with the day of the week, and might fall on any day, being regulated by the moon. That it fell in A.D. 31 on a Tuesday proves that that could not have been the year of the Crucifixion, which was probably A.D. 30, or possibly (as Prof. Sanday now thinks) A.D. 29. The seventh day of the week (the Jewish Sabbath) was the day after the Crucifixion, and the day after that, the first day of the week, the day of the Resurrection. Christians observed that day as their sacred day every week, and the Church decided (after the early Quartodeciman controversy, settled by the Council of Nicæa) to keep the Christian Passover (which we call Easter) always on that day of the week. There has, then, never been any break in the continuity of the week.

Blackheath, May 13. W. T. Lynn.

The Rusting of Iron.

In view of the correspondence which has taken place recently in Nature on this subject, I should like to refer to the results of some further investigations which I have made in conjunction with Mr. J. R. Hill in continuation of those published in the Journal of the Chemical Society in 1905. In that paper, and in other previous publications, experimental evidence was brought forward to show that the rusting of iron can take place in the absence of carbon dioxide, contrary to the generally accepted view. Several chemists have addressed themselves to the task of defending the old opinion that carbon dioxide is necessary. Their arguments were summarised recently in an article in Nature initialled "T. M. L." No exception can be taken to this article if its limitations are clearly recognised. It omits all reference to the large body of experimental work which has been published by Whitney, Tilden and others, in addition to myself, to show that the old view must be abandoned. The most recent work of Lambert and Thomson confirms this conclusion, whilst making an important addition to our knowledge of the conditions of reaction between iron, oxygen, and water when brought together in the most highly purified forms.

My object in writing this note is to state that I have now ascertained the cause of the inhibiting effect which certain substances, including alkalis and potassium bichromate, exert in preventing the rusting of iron, and it therefore becomes possible to explain a number of facts, including certain results which have been held to prove that carbon dioxide is a necessary factor in rusting.

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